

ivers and other bodies of water of the various continents, by Prof. H. Fritz. The author does not think there is any reason for believing that anything like a permanent decrease of the volume of water in rivers has taken place, but that this volume is subject to variations, which, when grouped in periods of about ten years, are seen to be wonderfully regular. He gives, for example, the years 1804, 1816, 1829, 1837, 1848, 1860, 1871, as years of water maxima, and notes as at least a coincidence that these were years of maximum sun-spots. An article by P. F. Bainier refers to the recent discovery of the Niger sources; there is information on various recent Nile expeditions, and some notes in connection with the projected railway from Mejillones to La Paz in Bolivia.

ARTIFICIAL DIAMONDS¹

IN a preliminary notice, which the Royal Society has done me the honour of publishing in the *Proceedings*, I gave a very short sketch of the work I have done which led me to a reaction whereby hard crystalline carbon has been produced. I have now the honour of laying a detailed account of the methods and results before the Society. As far back as September, 1879, I was searching for a solvent for the alkali metals, and tried experiments with many liquids and gases, but invariably found that when the solvent reached the permanently gaseous state chemical action ensued. This was the case even with hydrocarbons, the metal combining with the hydrogen and setting free the carbon. Paraffin spirit, boiling at 75°, was first used in experimenting, and the spirit contained a considerable amount of olefines; but even these unsaturated hydrocarbons seemed to be split up in like manner. The experiments were conducted in thick tubes from 1 to 1.5 millims. internal, and 10 to 15 external diameter, and made of hard glass.

The alkali metal which decomposes the hydrocarbon retains a quantity of pure hydrogen, which may be seen by exhausting it by the Sprengel pump. A piece of sodium was exhausted in the molten state for five hours by the Sprengel pump, and when no more hydrogen had been evolved for an hour, a piece was placed in a tube with paraffin spirit and heated for two hours, and when a considerable quantity of carbon was deposited, as much of it was removed as could be conveniently obtained and again exhausted, when 32 times its volume of hydrogen was extracted from it. This was repeated several times, and quantities of hydrogen, varying from 17 to 25 times the volume of the sodium, obtained. The carbon deposited on the tube is of a hard scaly nature, and when the sodium is slowly oxidised and dissolved in water, some very hard scales of carbon are often obtained. This was then the reaction on which my work was built. As potassium is a metal of stronger affinities I thought that an examination of its action on paraffin would yield somewhat better results, but in this I was disappointed. Sometimes its action was very great, but it seemed to combine with some of the substance in the tube, and formed black compounds, having no hard carbon amongst them. Some of the experiments did yield a little, but on the whole it was not so good as sodium. Lithium was next tried, and yielded results which were much more hopeful.

After an account of experiments on gaseous solution the author proceeds:—The general result obtained from these experiments was that the solvent power of water was found to be determined by two conditions: 1. Temperature or molecular *vis viva*; and 2. Closeness of the molecules on pressure, which seems to give penetrative power. From these observations it will be seen that if a body has any solvent action on another and does not act upon it chemically, such solvent action may be indefinitely increased by indefinitely increasing the temperature and pressure of the solvent. In nature the temperature has been at one time higher than we can obtain artificially, and the pressure obtained by a depth of 200 miles from the surface is greater than can be supported by any of the materials from which we can form vessels. It will thus be seen that, whereas in nature almost unlimited solvent power could be obtained, we are not as yet able to reproduce these conditions artificially. Could pressure alone increase solvent power, then much might be done, but pressure only acts by keeping the molecules close together when they have great *vis viva*, and this latter is only obtained by high temperature.

As glass tubes were quite out of the question when a red heat

and very high pressure were required, iron tubes were resorted to, and a series of attempts made to dissolve carbon by various gaseous solvents. The difficulty of closing iron tubes as compared with glass tubes caused me to try various methods, which I shall describe here. Tubes were made of strong hydraulic tubing 20" long, 1" thick, and $\frac{1}{2}$ " bore. These were fitted with a plug, screwed with a strong screw fitting very well. There was placed in the tube some powdered charcoal from which all the inorganic matter had been removed by immersion in hydrochloric and hydrofluoric acids and washing with water, and then sufficient paraffin spirit to fill the tube two-thirds of its volume. The plug was screwed in with a lute composed of silicate of soda and manganese dioxide, but after heating the tube in a reverberatory furnace for four hours it was found to be impossible to remove the plug, so the end had to be bored out. There was neither liquid nor gas in the tube, the luting having leaked. Another tube similarly filled was fitted with a plug with a copper washer, the end of the tube, plug, and washer being polished, but this also leaked, and no result was arrived at. Baryta, clay, asbestos, and other substances, wet with silicate of soda, were all tried with the same result—leakage. A silver washer kept comparatively tight, but only on one occasion. It was thus seen that screw closing would give no reliable results, so another method was tried. A ball of iron, fitting the tube tightly, was placed in it after the materials had been introduced. The end of the tube was then narrowed by compression between rollers and turned smooth inside. The iron ball was then drawn up by a wire attached and luted by silicate of soda and fine manganese dioxide. It was expected that the pressure would only serve to make the closing more secure, but, on heating, the iron yielded, and the ball was driven out with a loud explosion. After trying several other methods of closing—outside screwing and filling the mouth with molten metal on the top of a clay plug being amongst them—I came to the conclusion that nothing would suffice but welding up the open end. This has been, when carried out efficiently, invariably successful, and in all my later experiments I have used it alone. It requires great skill on the part of the workman, and it is only one man in a hundred who can perform the operation with invariable success. The furnace used in these experiments was a reverberatory one, 6 feet long (internal measurement) and 2 feet broad; fire-place, 15 inches; bridge, 9 inches; hearth, 4 feet. The roof sloped down towards the flue, and the spent gases had exit at the level of the hearth, thus carrying the flame down as it receded from the fire in order to have the hearth of one temperature. The walls were 13 inches thick, and the roof formed of 4-inch fire-clay covers.

Three tubes, 20" × 1" × $\frac{1}{2}$ " bore, were filled as follows:—

No.	I.	3 grms. sodium,	$\frac{3}{4}$ full paraffin spirit.
"	II.	" "	" "
"	III.	" "	" "

On heating them in the reverberatory furnace, No. I. exploded before a visible red-heat had been obtained, so the temperature was not allowed to rise any higher, and Nos. II. and III. allowed to lie for four hours and then slowly cooled. On being bored open next day, No. II. contained a little scaly carbon, but No. III. contained almost none, and nearly all its liquid had been converted into gas, which rushed out on boring it open. It was noticed by the workmen that the inside of the tube was harder to bore than the outside, and I thought, as I found out afterwards rightly, that the iron had been carbonised and converted into steel. It seemed, then, that the free carbon had been taken up by the iron.

An account of a number of preliminary experiments with various tubes here follows:—The iron used in making the tubes is what is known as "Lowmoor" iron, a very pure and strong quality, and a portion removed from the interior of a tube which has been used gave, on analysis, 2.17 per cent. of carbon, showing to what an extent carbonisation had gone on.

Having obtained results from this process of a kind which showed that diamond was unlikely to be formed by its agency, I reverted to the original idea of solution of carbon in a gaseous menstruum, and from some experiments I had been carrying on with the view of finding some commercial use for "bone oil," I concluded that the distillate from bone oil containing the nitrogenous bases would be most likely to yield such a solvent. Bone oil, the nitrogenous distillate obtained in the manufacture of bone char, and for a plentiful supply of which I am indebted to Messrs. John Poynter and Sons of Glasgow, was distilled, and the portion boiling between 115° and 150° was taken and rectified

¹ "On the Artificial Formation of the Diamond." Paper read at the Royal Society by J. B. Hannay, F.R.S.E., F.C.S. Abstract by the Author.

over solid caustic potash, and latterly over sodium. When satisfied that it was free from moisture, oxygen, and sulphur, a tube, $2\frac{3}{4}'' \times 20'' \times \frac{1}{8}''$ bore, was three parts filled, and some charcoal powder added, and the whole welded up solid. I found that the nitrogenous liquid was even worse to work with than the hydrocarbon, as on coming into contact with the hot iron it burnt it away at once, and as the tube was of great diameter it was extremely difficult to keep the lower part cool. For welding it had to be arranged so that it was standing in a tub of ice, and the top projecting through the bottom of the forge, and heated until it was at a welding heat, with as little delay as possible. When a tube was obtained welded up solid it was heated to a dull red-heat for 14 hours and allowed to cool; on opening the tube there was a very great out-rush of gas, and the carbon was to a certain extent dissolved, and some minute portions of it very hard. Still, under the microscope it presented little difference in appearance from the wood charcoal employed, some of the features, however, being obliterated, and it had a bright appearance. Another tube of the same dimensions and contents was closed up in the same manner, but after eight hours' heating it burst with a loud explosion. I had noticed that a tube which had been once used and been partially carbonised would not stand a second heating, and for this reason I had no belief in the power of cast-iron or steel to withstand the great pressure at a red heat. Nevertheless, as many of my friends had urged upon me to try these materials, I had a cast-iron tube made, $3\frac{3}{4}'' \times 24'' \times \frac{1}{8}''$ bore, and filled two-thirds of its volume with bone oil distillate and carbon, and then welded up. We succeeded after a little trouble in making a good weld, and the tube was then slowly raised to a dull red-heat in the furnace. It had not been heated for more than an hour when it exploded with a great noise and knocked down the back and one of the ends of the furnace, leaving the whole structure a wreck. The tube had broken into small fragments, and was quite unlike the malleable iron tubes which generally tore up. Thinking that it was perhaps a bad casting, I tried another, but it leaked all over, and emptied itself before the temperature was nearly up. A third tube of the same material burst like the first, but as I had built up the furnace with large blast-furnace blocks, it was not blown down. Cast-iron being inadmissible, experiments were then made with steel. I had several tubes made of this material by the best firms in the kingdom—made by the three methods, Bessemer, Siemens, and the crucible method—but they had the same faults as cast-iron, although to a less degree. The difficulty in making a good weld in cast-iron and steel tubes makes their employment in such experiments as these a matter of inconvenience. Out of five tubes made of steel, some of which were made of the very toughest material manufactured by Messrs. Cammell and Co., only one held in the substance completely. Three burst in the furnace, and one had leaked by its porosity. The top of the furnace, by the continued shocks of explosions, fell in at the bursting of the last of the steel tubes. The continued strain on the nerves, watching the temperature of the furnace, and in a state of tension in case of an explosion, induces a nervous state which is extremely weakening, and when the explosion occurs it sometimes shakes one so severely that sickness supervenes. An account of several experiments follows, none of which were, however, successful.

I thought I should either have to abandon the attempt or begin experiments of a very expensive nature, using large tubes and a large furnace, as 20-inch tubes of a greater diameter than four inches could not be closed when three parts filled—at least by welding. As some of them, however, seemed to stand, I determined to make some further trials with the apparatus I had at my disposal; so another tube, $20'' \times 4'' \times \frac{1}{8}''$ bore was filled, using 4 grms. of lithium and a mixture of bone oil, carefully rectified, 90 per cent., and paraffin spirit 10 per cent., using these proportions because I had never had any results with a high percentage of bone oil, the tubes so filled having burst. The tube was closed with great difficulty, being three-parts full of liquid, and then heated to a visible red heat for fourteen hours, and allowed to cool slowly. On opening the tube a great volume of gas was given off, and only a little liquid remained. In the end of the tube which had been the upper end in the furnace, the tube lying obliquely, there was a hard smooth mass adhering to the sides of the tube, and entirely covering the bottom. As I had never obtained all the solids in one piece before, I wished to examine it, and so had the other end of the tube cut off, exposing the hard mass. It was quite black, and was removed with a chisel, and as it appeared to be composed principally of iron and lithium, it

was laid aside for analysis. I was pulverising it in a mortar when I felt that some parts of the material were extremely hard—not resisting a blow, but hard otherwise. On looking closer I saw that these were mostly transparent pieces imbedded in the hard matrix, and on triturating them I obtained some free from the black matter. They turned out to be crystalline carbon, exactly like diamond. I shall describe further on the analyses, &c., but will here go on with the account of my further experiments. Two tubes were filled in the same manner as the last, but one burst on heating, and the other had leaked so that there was no reaction. Two more tubes were prepared, but were spoiled on welding, and on cutting off the carbonised portion the remainder was too short to work. After much trouble three tubes were obtained, well closed, in which the three alkali metals were inclosed with liquid containing 20 per cent. bone oil and 80 per cent. paraffin. All three stood, and, on opening, only the potassium one had leaked to any extent. The results were not good, however, the sodium tube containing only soft sealy carbon, and the other two very little better. The reaction did not seem to have proceeded in the same manner in the lithium tube as before, as the mass was soft and friable. Still, lithium seemed to yield the best results, so it was adhered to in the further experiments. A list of disasters now awaited me. Eight tubes failed through bursting and leaking, and one of the explosions, when two were being heated together, destroyed a part of the furnace and injured one of my workmen. Besides this, two tubes were spoiled in welding. However, I had four experiments after this, all withstanding the pressure, and in one of these, with 10 per cent. bone oil and 90 per cent. paraffin spirit, a small quantity of diamond was found. The contents of this tube were different from the other successful one, being much looser and not in the same hard mass as the first. In another series of six experiments two were at first thought to have been successful, but I afterwards found that one of them was not so, the transparent matter being siliceous, but insoluble in cold hydrofluoric acid, although it dissolved on boiling. The uncertainty and great expense involved in using these forged coils of iron with tubes bored out of the solid induced me to again try steel, and Messrs. Cammell and Co., having prepared some tubes for me, I tried them, but with the same results—they exploded into fragments at a red heat. And herein they are much more dangerous than coiled tubes, because the latter seldom fly into fragments, but just tear open a little. A further unforeseen danger in using steel tubes was discovered. One which had stood the heating very well was being bored, and when the inner skin was cut so that the gas rushed out, the whole exploded, endangering the life of the workman who was boring, but as he was standing at the end of the tube and the pieces flew laterally, he was not hurt. I have performed over eighty experiments, and have only obtained three results of a successful nature. The identification of the crystalline pieces as carbon was easy enough, but I have been anxious to find whether they are pure carbon or a compound with some other element, and to that end the following experiments were conducted.

A portion of the substance from the first successful experiment was weighed out after it had been freed from all foreign matter adhering to it, and placed in a very small platinum boat made of a strip of thin foil, the ends of which were wrapped round two stout platinum wires which were sealed into a wide glass tube. The carbon particles were transferred to this boat after being weighed, and the tube connected by india-rubber stoppers with an oxygen gasometer on the one side and a series of potash bulbs on the other. The oxygen was dried over solid caustic potash before entering the tube, and again after leaving the potash bulbs. The carbon (14 mgrms.) having been weighed out, the potash bulbs were weighed, and a current of oxygen passed through the apparatus, and the platinum wires connected with a battery strong enough to heat the foil to a bright red-heat. After a few minutes the oxygen was stopped and the bulbs weighed, when it was found that they had gained 1 mgrm. On repeating this operation no gain was found, the moisture having been entirely driven off by the first treatment. The carbon was now placed in the boat, and a slow current of oxygen started, then the bulbs connected and the current made to pass through the platinum until all the diamond had been burnt, when the current was stopped and the oxygen allowed to pass for fifteen minutes more, when the bulbs were detached and weighed. They were then reconnected and the gas passed for other ten minutes to find whether all the carbonic acid had been expelled,

and reweighed. They weighed 0.2 mgrm. less than before. The numbers were as follows:—

Potash bulbs before combustion	...	43.8308	
„ „ after „	...	43.8776	
			0.0468
Drying tube before combustion	...	26.4294	
„ „ after „	...	26.4328	
			0.0034
			0.0502

This gives a composition of 97.85 per cent. of carbon, which is a pretty fair approximation to pure carbon. However, to determine whether or not this was the case, some further experiments were tried. A small quantity of the carbon was placed on the platinum boat and burnt in oxygen without any of the gas being allowed to pass out of the apparatus, and the mixed gases so obtained transferred to a eudiometer, and the carbonic acid and oxygen absorbed. It was then found that a residue amounting to about 3 per cent. of the carbonic acid was left unabsorbed by alkaline pyrogallate solution. This proved to be nitrogen. A blank experiment was done, but it gave only a minute bubble of nitrogen. Another experiment was performed with the following results:—

Total volume	...	183.7	
After absorption of CO ₂	...	148.5	CO ₂ = 35.2
After „ O	...	11.1	147.4
		N	1.1

This plainly shows that nitrogen was present from some cause or another, and as every precaution was taken in transferring the gas from one vessel to another, and as the blank experiment showed nothing, I am inclined to believe that the carbon, or at least some portions of it, contained nitrogen chemically combined. The numbers above given are degrees on the eudiometer tube and are not more than one-third of a cubic centimetre each. Their exact value was of no consequence in the experiment, and the tube was only calibrated by comparing one part with another, and not with an absolute measure.

From the fact that no diamond was found when nitrogen compounds were absent, and from the fact that the mixed product (for only a portion of the 14 mgrms. was clear diamond) contains nitrogen, I am inclined to believe that it is by the decomposition of a nitrogenous body, and not the hydrocarbon, that the diamond is formed in this reaction. The experiments are, however, too few, and the evidence too vague, to draw any conclusions, as there are even very few negative experiments from which anything can be learned, most of the results being lost by explosion. I intend, when my other work—which I laid aside for the diamond experiments—is finished, to begin a series of experiments on the decompositions of carbon compounds by metals, to find whether a more easily-controlled reaction may not be discovered.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

THE following circular has been issued by the Science and Art Department:—"It having been represented to the Lords of the Committee of Council on Education that many parts of the kingdom are still in ignorance of the system of aid to the formation of classes for instruction in the principles of agriculture afforded by the Science and Art Department; that the supply of teachers who have obtained the necessary qualification to earn payments on results is very limited; and that a strict adherence to the rules of the Science Directory, which require that, in order to obtain aid, classes must be under the instruction of such teachers, would entail the delay of a year in the commencement of classes in this important subject, my Lords decide that §§ xxxiv. and xxxvi. of the Directory may be relaxed for this year in the following manner:—My Lords will be prepared to consider an application from any committee, formed in accordance with § x. of the Science Directory, to grant a temporary qualification to any person selected by it as fitted to teach the principles of Agriculture, and, if such application be found satisfactory, will permit the teacher to earn payments on the results of the examination in May, 1881; on the condition that this provisional qualification shall then determine, and that the only teachers who can after that date be recognised as qualified to earn payments on the results of their teaching in this subject will

be such as have complied with the ordinary rules. In making the application the committee must show that there is no technically qualified teacher in the locality who could be employed to instruct the class, and also state the grounds on which the proposed teacher is considered to be really capable of giving instruction in agriculture, by his knowledge of chemistry and other sciences bearing on the subject."

MR. RICHARD CHARES ROWE, M.A., B.Sc., Fellow of Trinity College, Cambridge, has been appointed Professor of Mathematics in University College, London.

PLANS have been prepared for a new botanical class-room in connection with Edinburgh University, the present room being much too small. The plans have been submitted to Government; if approved there will be a grant for the purpose required. The new class-room proposed will be seated for six hundred students, while the old class-room will be altered so as to be used as a practical and histological class-room.

SCIENTIFIC SERIALS

American Journal of Science, June.—Physical structure and hypsometry of the Catskill Mountain region, by A. Guyot.—Recent explorations in the Wappinger Valley limestone of Dutchess Co., N.Y., by W. B. Dwight.—The colour-correction of certain achromatic object-glasses, by C. A. Young.—Note on the companion of Sirius, by A. Hall.—Study of the Emmet Co. meteorite that fell near Estherville, May 10, 1879, by J. Lawrence Smith.—Oxidation of hydrochloric acid solutions of antimony in the atmosphere, by J. P. Cooke.—Relation between the colours and magnitudes of the components of binary stars, by E. S. Holden.—Occurrence of true lingula in the Trenton limestones, by R. P. Whitfield.—Experiments on Mr. Edison's dynamometer, dynamo-machine, and lamp, by Profs. Brackett and Young.—On substances possessing the power of developing the latent photographic image, by M. Carey Lea.

Archives des Sciences Physiques et Naturelles, June 15.—Researches on the temperature of Lake Lemane and other freshwater lakes, by Prof. Forel.—The disease of workmen employed in the St. Gothard tunnel, by Dr. Lombard.—Explosions by freezing, by Prof. Hagenbach.—On a yellow rain observed near Bonneville in Savoy, on April 25, 1880, by M. de Candolle.—Diatoms of the Alps and the Jura, and of the Swiss and French region in the environs of Geneva, by M. Bonn.—On a simplification of the theory of vibratory movements, by M. Cellérier.

Atti dei R. Accademia dei Lincei, fasc. 6, May.—Distribution of electricity in equilibrium on two parallel indefinite plane conductors, subjected to the induction of a point in the space included by them, by Dr. Maggi.—On a meteoric rain, containing an abundant quantity of metallic iron, observed at Catania on the night of March 29-30, 1880, by Prof. Silvestri.—On bromo-camphor, by Prof. Schiff.—Chemical and pathological studies on the hematopoietic function, by SS. Tizzoni and Filletti.—Influence of light on the production of hæmoglobin, by the same.—On ethylnaphtaline, by S. Camelutti.—On phenol derived from santonosic acid, by the same.—On a connection between meteorological phenomena and the time of arrival of the earth at perihelion, by Mr. Jenkins.—On the electric polarisation produced by metallic deposits, by Prof. Macaluso.—On the envelope and structure of the uveal tract in vertebrates, by Dr. Angelucci.—Helminthological observations on the endemic malady of the workmen in the St. Gothard (*Anchylostoma duodenalis*), by Prof. Perroncito.

Reale Istituto Lombardo di Scienze e Lettere. Rendiconti. Vol. xiii, fasc. xii.—On the aberration of sphericity, &c. (continued), by Prof. Ferrini.—On injury to agriculture caused by the winter 1879-80, by Prof. Cantoni.—On a problem of electrostatics, by Dr. Maggi.

La Natura, vol. iv, Nos. 3 and 4 (February).—On some recent studies in agrarian meteorology, by S. Porro.—Morphogeny of animal individuality, by Dr. Cattaneo.

Bulletin de l'Académie Royale des Sciences de Belgique, No. 4, 1880.—Letter from Dr. Huggins on the subject of M. Fievez's recent note.

Journal de Physique, June.—Vibrations on the surface of a liquid in a rectangular vessel, by Prof. Lechat.—On the economic yield of electric motors, and on measurement of the quantity of energy which traverses an electric circuit, by M.